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New Energy Times Archive

QUESTIONS PROVOKED BY THE EXPERIMENTS

by

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(also
Adjunct Professor of Physics,
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April 12, 1989

Presented at
Ettore Majorana Center for Scientific Culture
Erice, SICILY

1.0 QUESTIONS PROVOKED BY THE EXPERIMENTS.

1.1 BRIGHAM YOUNG UNIVERSITY-- ARE NEUTRONS GENERATED BY ELECTROLYSIS?

This is a crucial question.

1.1.1 If so, how reproduce and optimize?

1.1.2 Why not vast amounts of data since March 23, 1989?

Date: Sun, 14 May 89 11:23:15 PDT
From: koonin@sbitp.bitnet
Message-Id: <890514112316.6b6@sbitp.ucsb.edu>
Subject: note via John schiffer on cold fusion in italy
To: rlg2@yktvmv.bitnet, nauenbe@ucscd.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet", ST%"nauenbe@ucscd.bitnet", DOUG

From: ST%"schiffer@anlphy.bitnet" 13-MAY-1989 08:16
To: KOONIN
Subj: RE: reort on the ecs meeting

Received: from ANLPHY by sbitp.bitnet via BITNET ;
Sat, 13 May 89 08:16:17 PDT
Received: From ANLPHY(SCHIFFER) by SBITP with Jnet id 0428
for KOONIN@SBITP; Sat, 13 May 89 08:16 PDT
Message-Id: <890513081617.9a@sbitp.ucsb.edu>
Date: 13-MAY-1989 10:13:05.52
From: "SCHIFFER@ANLPHY (312)972-4066 FAX:972-3903" <SCHIFFER@ANLPHY>
To: KOONIN@SBITP.BITNET
Subject: RE: reort on the ecs meeting
X-ANJE-To: ANLPHY::GATEWAY::"KOONIN@SBITP.BITNET",SCHIFFER

Steve Koonin, enclosed is a note from Steve Pieper that I just received on a 'Cold Fusion Meeting at Trieste'. Note that the Frascati effect seems to have disappeared but a new Trieste effect is emerging.

Regards, John

=====

Date: Sat, 13 May 89 10:56 N Subject: fusion To: schiffer@anlphy

We had about an one hour session on cold fusion at the ICTP on Friday, May 12. By far the most interesting presentation was by Demanin who reported work done with Tommasini here in Trieste. They used Niobium cylinders 3/8" (10 mm) in diameter and weighing 35 grams. They first heated them up to 185C in 66 minutes to remove gas from them. Then they introduced D2 gas at 30 atm and continued heating as as follows

time	temp
46 min	211C
50	218
58	235
65	all done.

At this stage he had a 1:1 D:Nb ratio in the "beta" phase. They then returned to room temp and then to liquid N2 temp. Everything else was done at LN2 temp, it does not work at room temp according to him. So far they have used only 5 cylinders.

Their detector is a spectrometer for recoil protons. They have a proportional counter (NE213?) .

[note added by JS: NOT proportional counter -- NE213 is a liquid scintillator recoil used for neutron detection by proton recoil]

They have two channels. one discriminates recoil electrons (from gammas) from the recoil protons (due to neutrons). They monitored the stability by having a LED illuminate the phototube and then adjusting the amplifier gain. They see no bursts, just slowly changing signals .

He showed a typical proton recoil spectrum. His channel 0 was 1.5 MeV and channel 12 was 2.5 MeV. The spectrum shown was a difference of 30 min of having the reactor in front of the detector and 30 min nothing then reactor again and so forth for about a day I think. The spectrum had about 50 counts/channel in bins 0-6 and about 100 ± 40 (his error bars) in bins 6-12. above 12 it quickly dropped to 0. I gather this is reasonably close to the constant spectrum at energies less than the incoming neutron energy that one would expect. The detector was 5 cm in diameter by 5 cm long. Ingo Sick thought it looked like a reasonable neutron signal.

He also showed a spectrum from the other channel that showed an enormous gamma peak but a cleanly separated proton peak. Background was measured in two ways: 1) move reactor 1 meter away. 2) put reactor with H instead of D in front of detector. Both of these were alternated with the D-reactor in 30 minute intervals. Both give the same results, i.e. excess neutrons for the D reactor compared to either background.

For the first 2 hours after charging the cylinder, they see nothing. Then the counts start and they continue for 8 - 48 hours (different runs, they do not know why they last different times).

The signal is .3 neutrons/min compared to 2 n/min background. I also wrote down 50 n/hour signal so I don't know.

[note added by JS: for 8 hours of alternating between three setups this would correspond to about 320 counts in each of the two 'background' modes and 48 counts more in the 'signal' mode. For 48 hours it would be about 2000 counts each for 'background' and 300 more 'signal'. This would give something like 300 ± 60 as a signal -- consistent with the 5 std dev effect mentioned below.]

They have 1/3 of 4 pi coverage. Something is 36% efficient that neither Ingo or I understood. However he did say that after correcting for all efficiencies, the reactor was making 1600 ± 300 n/hour compared to 10,000 n/hr background.

They looked for alphas but did not see them??? (sorry I did not really catch what he said) they charged one 5 gm sample up to a 2:1 D:Nb ratio and saw no excess neutrons.

After a sample is used once, it is no longer any good. if they recharge it they do not get neutrons. Ingo thought this was good evidence that micro cracks were being made and acting as an accelerator. They objected that in a metal you cannot sustain such voltages as you can in other materials -- see below.

There were other shorter reports as follows:

Prof Barschal (?) from Moscow: 1) Kuzmin at Moscow State U saw neutrons by April 2. 2) Turinoff sees neutrons in a non reproducible way.

Someone from Darmstadt reported that Richter with WIPF has used the Fleishman setup. they looked for protons instead of neutrons. They got a negative result that will be published in Zeit. fur Kernphy. Initially they had a 6 sigma effect but it went away.

Someone else from Moscow reported that in 1986 Nature rejected a paper by A. V. Smilga that reported that when D2O ice pellets were smashed with a "special gun", they got 10-15 neutrons/shot. LiD gave same effect. In ice cracks one can get $1e6$ - $1e7$ volts/cm. from this they conclude a fusion probability of $1e-11$ /deuteron and this is reasonable from the 10 neutrons

they see. It is published in kolloidny Jr xlviii, pg 12

There was an interesting report of Frascati.

Scarmutsa (sp????) saw what we have in the preprint from there. However it has never been reproduced because with all the TV crews , etc., out there, no one has time.

That expt was an "unofficial" one. It was followed by a full scale official one with lots of solid state people, etc. much money and equipment I gather. The official expt has not seen anything. It was not at all clear why they have time for the real expt but no time to just reproduce the first unofficial one.

steve

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05/23/19

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

18 MAY 89 10. 18

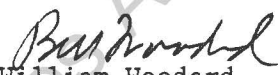
-R.L. GARWIN-

May 12, 1989

To: Cold Fusion Panel Members Attending May 23-25 Workshop in Santa Fe

Subject: Cost of Reception, Lunches, and Refreshments

The Workshop organizers have informed us that while they are still waiving the registration fee, they find it necessary to request reimbursement for the cost of the food at the scheduled reception, the lunches, and for the refreshments. This amounts to \$55 per registrant. Therefore, at registration you should expect to be asked for this amount. You can then include this in your travel voucher for reimbursement.


William Woodard
Panel Secretary

Addressees: Dr. Huizenga Dr. Ramsey
Dr. Bard Dr. Stein
Dr. Birnbaum Dr. Bigeleisen
Dr. Garwin Dr. Hoffman
Dr. Lipman Dr. Koonin
Dr. Wrighton Dr. Schiffer
Dr. Nelson Mr. Schoettler

05/12/89.

Ref brief NSB on col for redefining
the creation.

March 23, 1989.

S.E. Jones, et al (BYU) -- "neutrons" from a jet -- 170 ± 23 counts.
[in what] [10 hrs] --

M. Fleischmann, S. Pons, M. Hawkins (UA) --

- "Heat" a) explosive releases; b) 10 W/cm^2 for 100 hours

-- "tritium" -- "100 dpm/ml" vs $\sim 40 \text{ dpm/ml}$ in D_2O feed.

-- "gamma rays" -- $\sim 1000 \times 10$ counts in 50 hours (4000 ~~ran~~ / set/second).

-- He^4 -- "1-10 ppm" in D_2 off gas
[in what]

Press conferences Parth & Peter (1989)

Eric (Sully) ntg 04/12/89.

Am Phys. Soc ntg (Baltimore) 05/01-02/89.

E(rlinham Soc ntg (Los Angeles) 05/08/89.

The nay - finders

Brockhaven-Yale; IBM; Bell Labs; Horwell; Cal Tech (Kotelevski,
Charles Bowers, Steve Komin--); MIT Fusion Center --

The one - confiners

Georgia Tech, Hungary, Moscow, Beijing, U of Washington, ----

The maintainers

BYU, UA, Frascati?, Texas A&M;

05/12/89

(2)

Congressional response - Hearing 04/26/89; Sen Gore 05/01/89.

DOE response.

~~Depression~~
Communication

press release, FAX, BITNET, Express Mail.
Mullen physics

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170 cas
125

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05/11/89 czech

.qq
&EXIT

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If you have any questions, contact your manager.

The contents of the file has been shifted right by one character.

Filename=(none) Filetype=(none) RECFM=F LRECL=80 Records=58

The file received from the BITNET gateway begins below the next line.

Received: from CUNYVM by CUNYVM.BITNET (Mailer R2.01) with BSMTTP id 6069; Thu, 11 May 89 10:05:31 EDT
Received: from rocky2 by CUNYVM.CUNY.EDU (IBM VM SMTP R1.1) with TCP; Thu, 11 May 89 10:05:25 EDT
Received: by rocky2 (5.52/5.17)
id AA13393; Thu, 11 May 89 10:05:40 EDT
Return-Path: <robin@hcx>
Received: by hcx.ROCKEFELLER.EDU (5.51/HCX-2.2)
id AA02944; Thu, 11 May 89 10:03:31 EDT
Message-Id: <8905111403.AA02944@hcx.ROCKEFELLER.EDU>
To: rlg2@yktvmv.BITNET
Cc: robin@ROCKY2.ROCKEFELLER.EDU
Subject: Re: More mail trouble.
In-Reply-To: Your message of 11 May 1989, 08:46:30 EDT.
<8905111248.AA12787@rocky2>
Date: Thu, 11 May 89 10:03:30 -0500
From: Robin Bruckenstein <robin@ROCKY2.ROCKEFELLER.EDU>

The problem seems to be that your mailer does not conform to any RFC that I know of. When you add all the personal information in the ""To:" field it gets turned into "." seperated information. Here is an example of what gets delivered to me:

----- Message header follows -----
Return-Path: <rlg2@yktvmv.bitnet>
Received: from rocky2 by caspl.rockefeller.edu (3.2/SMI-3.2)
id AA01267; Wed, 10 May 89 22:09:04 EDT
Received: by rocky2 (5.52/5.17)
id AA08506; Wed, 10 May 89 22:10:22 EDT
Message-Id: <
(Mike Nauenberg).AA08506@rocky2>
Date: 10 May 1989, 21:51:19 EDT
From: (R.L.Garwin (914) 945-2555)
RLG2@YKTVMV.P.O.Box.218.Yorktown.Hts,
NY.10598@rocky2
To: (Douglas Eardley (805) 961-3979)
DOUG@SBITP.FPRESS@NAS.GUTZWIL@YKTVMV.KNEIFEL@SBCCMAIL.HLEWIS@SBITP.JBIGELEI@SBC
CMail.JSL@ROCKVAX.KOONIN@SBITP.RAMULLER@LBL.GOV.STEINH@PENNDRLS.WPRESS@CFA2.NAUE
NBE@UCSCD
(Frank Press
Subject: Comments on Koonin report of Electrochemical Society mtg.

As you can see this is not a legal "To:" field. I don't know what mailer you are using, but the standard meaning of "." is to

seperate domains within a hostname. Commas should be used to
seperate addresses.

One solution would be to try and use our internet address rather
than our bitnet address. Try using <jsl@rockvax.rockefeller.edu>.

I would also suggest you talk to the postmaster at your
site and see what he suggests. Sorry I can't be of
more help.

Robin Bruckenstein

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08/15/89
Copeh.qq
&EXIT

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Filename=(none) Filetype=(none) RECFM=F LRECL=80 Records=121

The file received from the BITNET gateway begins below the next line.

Received: from CUNYVM by CUNYVM.BITNET (Mailer R2.01) with BSMTTP id 9825; Tue, 09 May 89 18:29:20 EDT

Received: from rocky2 by CUNYVM.CUNY.EDU (IBM VM SMTP R1.1) with TCP; Tue, 09 May 89 18:29:13 EDT

Received: by rocky2 (5.52/5.17)

id AA19741; Tue, 9 May 89 18:29:28 EDT

Received: from rocky2 by caspl.rockefeller.edu (3.2/SMI-3.2)

id AB01107; Tue, 9 May 89 18:27:53 EDT

Date: Tue, 9 May 89 18:27:53 EDT

From: MAILER-DAEMON@caspl.rockefeller.edu (Mail Delivery Subsystem)

Subject: Returned mail: Unable to deliver mail

Message-Id: <8905092227.AB01107@caspl.rockefeller.edu>

To: <rlg2@yktvmv.BITNET>

----- Transcript of session follows -----

554 <rlg2@yktvmv.bitnet>... Unbalanced '('

554 <jsl@caspl.rockefeller.edu>... Unbalanced '('

554 <jsl@caspl.rockefeller.edu>... Unbalanced '('

----- Unsent message follows -----

Return-Path: <rlg2@yktvmv.bitnet>

Received: from rocky2 by caspl.rockefeller.edu (3.2/SMI-3.2)

id AA01104; Tue, 9 May 89 18:27:53 EDT

Received: by rocky2 (5.52/5.17)

id AA19730; Tue, 9 May 89 18:29:05 EDT

Message-Id: <

(Mike Nauenberg).AA19730@rocky2>

Date: 9 May 1989, 18:12:43 EDT

From: (R.L.Garwin (914) 945-2555)

RLG2@YKTMV.P.O.Box.218.Yorktown.Hts,

NY.10598@rocky2

To: (Douglas Eardley (805) 961-3979)

DOUG@SBITP.FPRESS@NAS.GUTZWIL@YKTMV.KNEIFEL@SBCCMAIL.HLEWIS@SBITP.JBIGELEI@SBCMAIL.JSL@ROCKVAX.KOONIN@SBITP.RAMULLER@LBL.GOV.STEINH@PENNDRLS.WPRESS@CFA2.NAUE NBE@UCSCD

(Frank Press

Subject: A hole in the Coulomb barrier?

On page 712 of NATURE of 04/27/89 is an excerpt of a letter from M. Gryzinski, who invokes a "collapsing quasi-molecule" to explain d-d tunneling and fusion. His example is an electron "at the centre-point between the two" deuterons.

I have drafted a reply to the three silly letters, but I was wanting to obtain some wisdom from Gordon Lasher here, who tells me that John Slonczewski

had asked him about the same configuration-- obviously the Coulomb energy is negative and becomes MORE negative as the configuration collapses. Here is what I had written:

I note a comment by a referee in Scientific Correspondence of 27 April 1989 (pages 711-712), together with "extracts of letters" on cold fusion. Here are comments that might have been made by a referee on the letters by P. Paolo (Oppenheimer-Phillips reaction), M. Gryzinski (collapsing quasi-molecule), and F. Premuda (super-dense regions in metal hydrides).

In the O-P process, nuclear reactions occur because of the exponential falloff of neutron wave function outside the range of n-p nuclear force, corresponding to a binding energy of 2.22 MeV and a nucleon reduced mass of 0.5 amu. This is significant only when the falloff of the wave function of the deuteron as a whole toward the target nucleus is stronger than this spatial decay of the neutron wave function in the loosely bound d. If the target nucleus has a mass substantially greater than 2 amu, the deuteron reduced mass is 2, and the O-P effect would be extremely significant for a Coulomb barrier of, say, 3.33 MeV, for which the exponential decay length of the deuteron near the target nucleus would be shorter than the decay length of the neutron skirt by a factor

(1)

$$((2 \times 3.33)/(0.5 \times 2.22))^{1/2}$$

or 2.45. However, for the d-d reaction, the deuteron reduced mass is 1, and the barrier is only 0.6 MeV, so that the wave function of the deuteron as a whole in the barrier falls off at worst only .73 times as fast as the neutron skirt. The O-P process is therefore unimportant.

As for a "collapsing quasi-molecule" with an electron midway between 2 deuterons, the author claims too little. If the molecule should scale as it collapses, with linear dimensions equal to the original ones divided by a factor f, the Coulomb energy increases by a factor f, but the electron kinetic energy (due to localization) increases by a factor f-squared. In fact, this is responsible for the continued existence of hydrogen atoms (and all others), and can hardly be ignored.

As for the super-dense regions in which d-d fusion might occur (F. Premuda), even qualitative analysis can eliminate this hypothesis. Deuterons on a lattice of spacing about 0.15 A would provide the claimed fusion rate of 10²⁰ per second per d (although no plausible means has been advanced to obtain such small spacing). A deuteron pair would create a single helium nucleus, with no "multiple scattering" of an outgoing particle. Several d in such a cluster would have a scattering probability which is only on the order of the square of the ratio of nuclear radius to mean separation of deuterons, or 10⁻⁸.

Those of your readers who are nuclear physicists can readily

calculate such matters themselves; perhaps you might publish such "referee comments" along with the conjectures themselves, if you publish the latter at all.

Gordon points out that my view of the d-e-d problem is conventional Born-Oppenheimer approximation, and that Feynman encourages us to consider all path integrals including one corresponding to this particular configuration. But he hasn't told me yet how to compute its contribution.

You have your assignment.

Go!

Dick Garwin

New Energy Times Archive

=====

Date: Tue, 9 May 89 12:03:56 PDT
From: koonin@sbitp.bitnet
Message-Id: <890509120356.3be@sbitp.ucsb.edu>
Subject: report on the ecs meeting
To: perry@ohstpy.bitnet, feng@duvm.bitnet, rlg2@yktvmt.bitnet
X-ST-Vmsmail-To: ST%"perry@ohstpy.bitnet", ST%"feng@duvm.bitnet",
ST%"rlg2@yktvmt.bitnet"

May 9, 1989
11:30 PDT

There follows an eyewitness report on the special session on Electrochemically Induced Cold Fusion sponsored by The Electrochemical Society last night in Los Angeles. There were about 2000 people in attendance at the Bonaventure Hotel. No cameras or recording devices were allowed in the hall, so what follows is a somewhat subjective and incomplete report based on my notes. I will also restrict my report to what I consider new information that was presented.

PROGRAM:

The scientific program of the meeting was:

- "Electrochemically-Induced Nuclear Fusion of Deuterium" (S. Pons and M. Fleischmann) 40 min.
- "Observation of Cold Nuclear Fusion in Condensed Matter" (S. E. Jones et al.) 25 min.
- "Thermal measurements of D-Pd and H-Pd Systems" (R. A. Huggins et al., Stanford) 15 min.
- "Observations of Heat Generation, Increased Tritium Concentration, and Enhanced Neutron Count in the Electrolysis of Deuterium Oxide on Palladium Cathodes" (U. Landau et al., Case Western Reserve) 10 min.
- "Mass Spectrometric Detection of Hydrogenic Species during Electrolysis of D2O at a Palladium Cathode" (E. Struve et al., University of Washington) 10 min.
- "Electrochemically-induced Fusion of Deuterium: The Search for Neutrons and Fusion Products" (J. Jorne et al., University of Rochester) 10 min.
- "Evidence for Excess Heat Generation During Electrolysis of D2O (Pd Cathode/Pt Anode) in LiOD - A microcalorimetric Investigation" (S. Srinivasan et al., Texas A&M) 10 min.
- "The Fleischmann-Pons Effect: Facts and Theory at an Early Stage of Investigation" (J. Bockris et al., Texas A&M) 10 min.
- "Calorimetry, Neutron Flux, Gamma Flux, and Tritium Yield from Electrochemically Charged Palladium in D2O" (N. Lewis et al., Caltech) 10 min.

These talks were followed by about an hour of questions from the audience to all of the speakers. The (by now obligatory) press conference followed.

PONS and FLEISCHMANN:

The most surprising part of the Pons-Fleischmann presentation was how little

things had changed. It was basically a rehash of the same material we've been seeing for the last month. All of the Tables and Figures from the published paper (J. Electroanal. Chem.) were shown, with no apologies. The 10**27 atmospheres was still there, as was the completely fictitious "Column C" with the arbitrary 0.5 V.

Pons mumbled alot and his voice dropped significantly as he was discussing the gamma line. Under intense questioning in the session and at the later press conference, they admitted that "The gamma peak is wrong". However, they kept emphasizing that they place greatest reliability on the calorimetry data and that they are setting up a GeLi spectrometer to do better than with NaI.

Nothing definitive was said about their 3H measurements or about the 4He data many of us had expected them to announce. Several groups (MIT and Sandia) offered to make rapid (3 day) analyses of small pieces of the Utah cathodes for fusion products. But P/F responded that they were in the process of long charging experiments and that they had already made "other arrangements" for such an analysis. Who was going to do it or how long it would take was not revealed.

Two new tidbits were thrown to the crowd. The first was a brief video of one of their cells bubbling away. They dumped some dye (phenolphthaleine, if I've spelled it right) in the top, and you could see it diffuse through the cell in something less than 20 seconds. As this is much longer than the thermal time constant, 1600 sec, they conclude that their cell was well-mixed, without temperature gradients. Of course, the real proof, namely a measurement of the temperature as a function of the vertical distance, was not presented, although they said they had the numbers. The second new item was the report of calorimetric data showing a "burst" of excess heat production of about 1 Watt lasting for about $2 \times 10^{*5}$ sec. and producing 4.2 MJ. This is roughly 50 times the Joule heating going to the cell.

There was no direct repsonse to many of the criticisms that were raised last week in Baltimore and I saw lots of prevarication and evasion in the questions they had to respond to. Fleischmann said he had to see full details published in a refereed journal before he could respond (talk about the pot calling the kettle black!) None of the pieces of hard data that are required to prove their claim were shown. I can't understand why they would be holding these back now, as their scientific priority is surely guaranteed. However, they did say at the press conference that the published paper has "lots of omissions and mistakes in it". This is, of course, hardly news.

JONES:

The new pieces of data presented were reports of

- 1) continuing experiments at BYU using electrolytes with Pd+, Li+, and H+/D+ ions in them. Still seeing 2.5 MeV neutrons at about 0.04/sec.
- 2) Ongoing experiments in collaboration with LANL using 3He proportional counters. Nothing yet announced.
- 3) Work in collaboration with Bologna in the Gran Sasso using liquid scintillators with pulse-shape discrimination. The neutron signal seen in a counter near the cell turns on and off with the cell current, while a counter remote from the cell shows nothing. They see a signal of about 50 neutrons per hour lasting for about 3 hours, before it turns off. There is some evidence that the energy is 2.5 MeV, but the energy resolution is poor. Overall, it's a 7 sigma effect.

My general impression is that Jones is carefully following up previous work and it's looking somewhat more positive than before.

HUGGINS:

Reported only calorimetry involving direct comparisons between light and heavy water cells. There was no direct stirring, but he did say gyroscopic motion of the whole apparatus (water bath included) was used to keep things well mixed. He reports that the heavy-water cells run consistently hotter than those with light-water, but there are lot's of reasons why this might be so and he didn't address them very carefully. Bottom line is a claim of for 15% excess energy production in a short (35 hr) run and up to 10 MJ/mole Pd in longer runs.

Huggins stressed that an important experimental consideration was the microstructure and interstitial content of the Pd. Thought it better to use cast vs. wrought samples and to arc melt them to drive out hydrogen that might be in there. Also emphasized the Carbon contamination was a no-no, so that graphite crucibles were out. However, it was pointed out to him that commercial suppliers use graphite containers in making their Pd.

LANDAU:

Emphasized that this was a progress report of preliminary results. Ran 4 cells including light/heavy water comparison, and a Pt cathode instead of Pd. Claims excess heat of 0.144 W (6 W/cm² Pd) at a current density of 255 mA/cm² with D2O but nothing (0.025W) with H2O. No tritium observed beyond that expected from electrochemical separation. Many neutron points at the 3-4 sigma level were presented, but nothing convincing.

STRUVE:

Reported on mass-spectrometry of gases evolved at a Pd cathode in 99.95% pure D2O but no(!) electrolyte. They saw A=2,3,4,5,6 signals with a quadrupole mass spec and got excited by the latter two peaks, which could be attributed to DT and TT molecules. However, a higher-precision instrument capable of distinguishing nuclear mass showed that A=5,6 were due to tri-atomic hydrogen ions. It seems to me that any experienced mass spectrometrists would have recognized this immediately, and not held a press conference. The only excuse is that they were graduate students, I guess.

JORNE:

Looked for neutrons (nice scintillation counter, carefully calibrated efficiency), gammas, and fusion products. Limit on the neutron rate was <0.5/sec. P/F would imply 4 X 10⁻⁴/sec, while Jones would imply 0.4/sec. The limit on the gamma rate is <4/sec.

In a search for fusion products, they looked for 3H in the Pd with a mass spectrometer. They say A=4 (DD molecules) and A=3 (HD, perhaps, or 3H) There is

■ some suggestion'' that they're seeing 3H. but they can't prove it. I don't know what's making this suggestion to them.

SRINIVASAN:

This and the following talk were clearly the most interesting in the session. He

reported on microcalorimetry. He is a professional in this business and the apparatus looked, to my untutored eye, like it was well thought through. He can

measure heat with confidence from 1 microWatt to 8 Watt. This sensitivity allows the use of small samples to cut charging time and they can also keep the current small. They ran Pd rods of 10 mm in length and 0.5 and 1 mm in diameter, as well as a 2 mm diameter sphere. The several electrolytes were LiOD, LiOH, and NaOD, with a volume of 5-6 ml. There were light water and Pt cathode controls.

Of 20 cells constructed, 1/3 showed excess heat production. Best was about 30 mW (10% more than the power in). They can see up to 20 W/cm² Pd for 10 hours at 300, 600, and 1000 mA/cm². However, none of their heat-producers showed any neutrons (next talk).

BOCKRIS:

Reported on neutron measurements by Kevin Wolf, a nuclear experimentalist. Two of 20 cells constructed were "live" and one of these reproducibly so. A scintillation counter was used. The neutrons change in a non-monotonic way with current and fall off in an R⁻² test in which the cell is moved away from the counter. The statistics were only fair, but somewhat better than the Jones data published in Nature. Best rate was 50 n/min.

Also interesting were the 3H measurements reported. Starting from a background scintillation count rate of 60-80 dpm/ml, they see the count rate rise upon beginning electrolysis to about 10⁻⁶ dpm/ml after a few hours! They know all about the possible chemiluminescence interferences and have been careful to neutralize before counting.

LEWIS:

Not much beyond what was said in Baltimore. Some runs in which the current was changed in steps were also reported, but overall, no radiation or heat production or tritium observed. A preprint submitted to J. Electroanal Chem. by G. Kreysa, G. Marx, and W. Plieth (the first from the Dechema-Institute, Theodor-Heuss-Allee 25, 6000 Frankfurt) goes through an analysis of the P/F calorimetry data similar to Lewis' and comes to similar conclusions, I believe (I've yet to read it in detail).

OVERALL ANALYSIS:

Cold fusion is not dead yet. P/F are clearly on the ropes and are possibly on their way down. Jones is looking pretty good and there may be real physics there; I'm almost motivated to do more calculations. The A&M stuff is quite interesting. The neutrons would confirm Jones; I don't know what to make of the calorimetry. Perhaps Nature (or the "successful" experimenters) will reveal more of their secrets in Santa Fe two weeks from now.

Steven Koonin
KONIN@SBITP.BITNET

Date: 10 May 1989, 21:51:19 EDT

From: (R.L.Garwin (914) 945-2555) RLG2 at YKTMV
P.O. Box 218
Yorktown Hts, NY 10598

To: (Douglas Eardley (805) 961-3979) DOUG at SBITP
(Frank Press (202) 334-2100) FPRESS at NAS
(Martin C. Gutzwiller (805) 961-3178) GUTZWIL at YKTMV
(Harold L. Friedman (516) 632-7898) KNEIFEL at SBCCMAIL
(Harold W. Lewis (805) 961-2670) HLEWIS at SBITP
(Jacob Bigeleisen (516) 632-7905) JBIGELEI at SBCCMAIL
(Joshua S. Lederberg (212) 570-8080) JSL at ROCKVAX

(Steven E. Koonin	(805) 961-3126)	KOONIN	at SBITP
(Richard A. Muller	(415) 486-7430)	RAMULLER	at LBL.GOV
(Paul J. Steinhart	(215) 898-5949)	STEINH	at PENNDRLS
(Prof. William Press	(617) 495-4908)	WPRESS	at CFA2
(Mike Nauenberg)		NAUENBE	at UCSCD

====> Body of the note kept in DOUG NOTEBOOK A0.

Date: 12 May 1989, 22:38:45 EDT

From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
P.O. Box 218
Yorktown Hts, NY 10598

To: (Douglas Eardley (805) 961-3979) DOUG at SBITP
(Frank Press (202) 334-2100) FPRESS at NAS
(Martin C. Gutzwiller (805) 961-3178) GUTZWIL at YKTVMV
(Harold L. Friedman (516) 632-7898) KNEIFEL at SBCCMAIL
(Harold W. Lewis (805) 961-2670) HLEWIS at SBITP
(Jacob Bigeleisen (516) 632-7905) JBIGELEI at SBCCMAIL
(Joshua S. Lederberg (212) 570-8080) JSL at ROCKVAX
(Steven E. Koonin (805) 961-3126) KOONIN at SBITP
(Richard A. Muller (415) 486-7430) RAMULLER at LBL.GOV
(Paul J. Steinhart (215) 898-5949) STEINH at PENNDRLS
(Prof. William Press (617) 495-4908) WPRESS at CFA2
(Mike Nauenberg) NAUENBE at UCSCD

====> Body of the note kept in DOUG NOTEBOOK A0.

Date: Sun, 14 May 89 11:23:15 PDT

From: koonin@sbitp.bitnet

Message-Id: <890514112316.6b6@sbitp.ucsb.edu>

Subject: note via John schiffer on cold fusion in italy

To: rlg2@yktvmv.bitnet, nauenbe@ucscd.bitnet

X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet", ST%"nauenbe@ucscd.bitnet", DOUG

From: ST%"schiffer@anlphy.bitnet" 13-MAY-1989 08:16

To: KOONIN

Subj: RE: reort on the ecs meeting

Received: from ANLPHY by sbitp.bitnet via BITNET ;
Sat, 13 May 89 08:16:17 PDT

Received: From ANLPHY(SCHIFFER) by SBITP with Jnet id 0428
for KOONIN@SBITP; Sat, 13 May 89 08:16 PDT

Message-Id: <890513081617.9a@sbitp.ucsb.edu>

Date: 13-MAY-1989 10:13:05.52

From: "SCHIFFER@ANLPHY (312)972-4066 FAX:972-3903" <SCHIFFER@ANLPHY>

To: KOONIN@SBITP.BITNET

Subject: RE: reort on the ecs meeting

X-ANJE-To: ANLPHY::GATEWAY::"KOONIN@SBITP.BITNET",SCHIFFER

Steve Koonin, enclosed is a note from Steve Pieper that I just received on a 'Cold Fusion Meeting at Trieste'. Note that the Frascati effect seems to have disappeared but a new Trieste effect is emerging.

Regards, John

Date: Sat, 13 May 89 10:56 N Subject: fusion To: schiffer@anlphy

We had about an one hour session on cold fusion at the ICTP on Friday, May 12. By far the most interesting presentation was by Demanin who reported work done with Tommasini here in Trieste. They used Niobium cylinders 3/8" (10 mm) in diameter and weighing 35 grams. They first heated them up to

185C in 66 minutes to remove gas from them. Then they introduced D2 gas at 30 atm and continued heating as follows

time	temp
46 min	211C
50	218
58	235
65	all done.

At this stage he had a 1:1 D:Nb ratio in the "beta" phase. They then returned to room temp and then to liquid N2 temp. Everything else was done at LN2 temp, it does not work at room temp according to him. So far they have used only 5 cylinders.

Their detector is a spectrometer for recoil protons. They have a proportional counter (NE213?) .

[note added by JS: NOT proportional counter -- NE213 is a liquid scintillator recoil used for neutron detection by proton recoil]

They have two channels. one discriminates recoil electrons (from gammas) from the recoil protons (due to neutrons). They monitored the stability by having a LED illuminate the phototube and then adjusting the amplifier gain. They see no bursts, just slowly changing signals .

He showed a typical proton recoil spectrum. His channel 0 was 1.5 MeV and channel 12 was 2.5 MeV. The spectrum shown was a difference of 30 min of having the reactor in front of the detector and 30 min nothing then reactor again and so forth for about a day I think. The spectrum had about 50 counts/channel in bins 0-6 and about 100 +- 40 (his error bars) in bins 6-12. above 12 it quickly dropped to 0. I gather this is reasonably close to the constant spectrum at energies less than the incoming neutron energy that one would expect. The detector was 5 cm in diameter by 5 cm long. Ingo Sick thought it looked like a reasonable neutron signal.

He also showed a spectrum from the other channel that showed an enormous gamma peak but a cleanly separated proton peak . Background was measured in two ways: 1) move reactor 1 meter away. 2) put reactor with H instead of D in front of detector. Both of these were alternated with the D-reactor in 30 minute intervals. Both give the same results, i.e. excess neutrons for the D reactor compared to either background.

For the first 2 hours after charging the cylinder, they see nothing. Then the counts start and they continue for 8 - 48 hours (different runs, they do not know why they last different times).

The signal is .3 neutrons/min compared to 2 n/min background. I also wrote down 50 n/hour signal so I don't know.

[note added by JS: for 8 hours of alternating between three setups this would correspond to about 320 counts in each of the two 'background' modes and 48 counts more in the 'signal' mode. For 48 hours it would be about 2000 counts each for 'background and 300 more 'signal'. This would give something like 300 +- 60 as a signal -- consistent with the 5 std dev effect mentioned below.]

They have 1/3 of 4 pi coverage. Something is 36% efficient that neither Ingo or I understood. However he did say that after correcting for all efficiencies, the reactor was making 1600+-300 n/hour compared to 10,000 n/hr background.

They looked for alphas but did not see them??? (sorry I did not really catch what he said) they charged one 5 gm sample up to a 2:1 D;Nb ratio and saw no excess neutrons.

After a sample is used once, it is no longer any good. if they recharge it they do not get neutrons. Ingo thought this was good evidence that micro cracks were being made and acting as an accelerator. They objected that in a metal you cannot sustain such voltages as you can in other materials -- see below.

There were other shorter reports as follows:

Prof Barschal (?) from Moscow: 1) Kuzmin at Moscow State U saw neutrons by April 2. 2) Turinoff sees neutrons in a non reproducible way.

Someone from Darmstadt reported that Richter with WIPF has used the Fleishman setup. they looked for protons instead of neutrons. They got a negative result that will be published in Zeit. fur Kernphy. Initially they had a 6 sigma effect but it went away.

Someone else from Moscow reported that in 1986 Nature rejected a paper by A. V. Smilga that reported that when D2O ice pellets were smashed with a "special gun", they got 10-15 neutrons/shot. LiD gave same effect. In ice cracks one can get $1e6$ - $1e7$ volts/cm. from this they conclude a fusion probability of $1e-11$ /deuteron and this is reasonable from the 10 neutrons they see. It is published in kolloidny Jr xlviii, pg 12

There was an interesting report of Frascati.

Scarmutsa (sp????) saw what we have in the preprint from there. However it has never been reproduced because with all the TV crews , etc., out there, no one has time.

That expt was an "unofficial" one. It was followed by a full scale official one with lots of solid state people, etc. much money and equipment I gather. The official expt has not seen anything. It was not at all clear why they have time for the real expt but no time to just reproduce the first unofficial one.

steve

Date: 16 May 1989, 19:56:52 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTVMV
P.O. Box 218
Yorktown Hts, NY 10598
To: (Steven E. Koonin (805) 961-3126) KOONIN at SBITP
cc: (Jacob Bigeleisen (516) 632-7905) JBIGELEI at SBCCMAIL
Subject: Cold Nuclear Fusion library for our panel.
Reply-To: RLG2 at WATSON

Steve, I have sent Bill Woodard by express mail today the following letter, together with the listed enclosures. For reasons of propriety, ethics, and IBM policy, I did not send him the very interesting reports received from you via BITNET, and of course not various manuscripts received by me in confidence as a referee.

If you are willing to have your 2 or 3 substantive BITNET communications reporting on Electrochemical Society meeting, rumors, etc., communicated to our colleagues, I'd be glad to do it for you. Let me know either way, please.

Dick Garwin

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

May 16, 1989

Dr. William L. Woodard
Cold Fusion Panel Secretary
Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, DC 20585

Dear Dr. Woodard:

Jake Bigeleisen suggested to me that I send you a copy of each of the items that I can share with you and the other panel members on our topic of interest. Here they are, in single-sided form to make them easier to reproduce.

They are organized by date, with the most recent on top.

I hope these will be of some interest.

I recommend that your office keep a list of such materials, and send them once a week to those panel members who have indicated that they want such. The rest might receive just a list of items available from your office, which they could

receive on request.

It would be a good idea to standardize on a system for identifying documents in brief-- you could use either the date by which you list them, together with the first name to appear in the item, or you could give each item a serial number.

In my own office, every document is identified by a ten-digit lognumber consisting of its date (without the slashes) appended to four characters, which may be initial of the author. Examples: 041089.SEJ, 050289.RDP, etc. If there are fewer than four initials, I use dots to substitute for the missing leading initials. You can see the scheme from the lognumbers on the items that I enclose.

I do not include BITNET reports from others. Steve Koonin may want to contribute some of his interesting reportage.

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

PAGE 2

I look forward to working with you on this activity.

Sincerely yours,

Richard L. Garwin

Encl:

05/09/89 "Fusion in a Jar: Recklessness and Brilliance"
by W.J. Broad in The New York Times. (050989.WJB)

05/09/89 VNET RLG to D. Eardley, F. Press, M.C.
Gutzwiller, H.L. Friedman, H.W. Lewis, J. Bigeleisen,
J.S. Lederberg, S.E. Koonin, R.A. Muller, P.J.
Steinhardt, W. Press, M. Nauenberg re a hole in the
Coulomb barrier. (050989.RLG)

05/02/89 "Gamma-Ray Spectra in the Fleischmann, Pons,
Hawkins Experiment" by R.D. Petrasso, et al, Plasma
Fusion Center, MIT. (050289.RDP)

04/20/89 "Pons and Fleischmann Withdraw Fusion Paper
From U.K. Journal" by R.L. Hudson in The Wall Street
Journal. (042089.RLH)

04/19/89 "Enhancement of Cold Fusion Rates by
Fluctuations" by S.E. Koonin (Submitted to Physical
Review Letters, April 19, 1989). (041989SEK1)

04/18/89 "Two Innocent Chemists Look at Cold Fusion" by
C. Walling and J. Simons, University of Utah.
(041889.CW)

04/18/89 "Electrochemical Experiments in Cold Nuclear
Fusion" by J.F. Ziegler, et al (Submitted to Physical
Review Letters, April 18, 1989). (0411889.JFZ)

04/12/89 R.L. Garwin foils at Erice meeting on cold
nuclear fusion. (041289.RLG)

04/10/89 "A Simple Model for Coherent DD Fusion in the
Presence of a Lattice" by P.L. Hagelstein, MIT.
(041089.PLH)

04/10/89 "Observation of Cold Nuclear Fusion in
Condensed Matter" by S.E. Jones, et al, Brigham Young
Univ. and J. Rafelski, Univ. of Arizona. (041089.SEJ)

04/07/89 "Cold Fusion in Isotopic Hydrogen Molecules" by
S.E. Koonin and M. Nauenberg (Submitted to Nature,

April 7, 1989. (040789.SEK)

04/00/89 "Evidence of Emission of Neutrons from a
Titanium-Deuterium System" by A. De Ninno, et al,
Frascati, Rome. (040089FRAS)

04/00/89 "Phonon Interactions in Coherent Fusion" by
P.L. Hagelstein, MIT. (040089.PLH)
03/20/89 "Electrochemically Induced Nuclear Fusion of
Deuterium" by M. Fleischmann and S. Pons (Submitted to
Journal of Electroanalytical Chemistry march 11, 1989).

(032089..MF)

04/00/68 "Pathological Science," by I. Langmuir.
(040068..IL)

10/22/26 "Über die Verwandlung von Wasserstoff in
Helium" by Von Fritz Paneth and Kurt Peters in
Vierzehnter Jahrgang. (102226.VFP)

RLG:jah:136%WLW:051689.WLW

Date: Tue, 16 May 89 17:05:07 PDT
From: koonin@sbitp.bitnet
Message-Id: <890516170507.ea7@sbitp.ucsb.edu>
Subject: micro-cracks
To: rlg2@yktvmv.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet"

May 16, 1989

Dick:

Re "microcracks", I've just finished an estimate that might amuse
you. If you take the standard dE/dx for deuterium in Pd or Ti (Ziegler's
tables) and the measured $d+d$ cross section, you get about 10^{-25} nuclear
reactions per incident 1 keV deuteron (i.e., the thick target yield). If you
say a 1 cm^2 crack exposes 10^{-15} deuterons, you get only 10^{-10} neutrons per
crack, far too small to be important.

Alternatively, you can ask how high the incident energy has to be to see 1
neutron in a 1 cm^2 crack; the answer is 4 keV. If you want to cut the crack
area to 10^{-4} cm^2 ($100 \mu\text{m} \times 100 \mu\text{m}$), you need 12 keV to get just 1 neutron. The
energy increases only slowly with the number of neutrons required, of course.

I conclude one needs 10-20 keV potential drops at the crack if this is going to
be at all viable. Such values might not be impossible (but are perhaps
implausible) in an insulator, but are hard to believe in a metal.

Comments?

Steve Koonin

P. S. The program for the Santa Fe meeting turned out better than I thought it
would. There are at least several not obviously wrong groups (A&M, Jones,
Frascati) claiming positive results, so that I think it doesn't even matter if
P/F don't show up. It will be interesting to see if the claims stand up under
close scrutiny.

Date: 17 May 1989, 06:39:19 EDT
From: (R.L.Garwin (914) 945-2555) RLG2 at YKTMV
P.O. Box 218
Yorktown Hts, NY 10598
To: (Steven E. Koonin (805) 961-3126) KOONIN at SBITP
Subject: Microcracks.

Reply-To: RLG2 at WATSON

Your numbers sound right, and that's what I have always thought. Consider a "pyroelectric" material with one charge species displaced asymmetrically from another lattice (like quartz). It has an enormous built-in field of $4\pi P$, where P is the volume density of polarization-- some 5×10^{22} charges per unit volume times 0.3×10^{-8} displacement. For $q = 4.8 \times 10^{-10}$ esu, this is 10^6 statvolts/cm or about 300 MV per cm. If a phase change is introduced by sudden heating or shocking with high explosive, this is the potential field, giving 20 kV for a thickness of only 0.7 micron of material.

The same would be true of cracking a material in which the internal field had been compensated by very large surface charges.

How does that grab you?

Dick Garwin

Date: Wed, 17 May 89 10:14:32 PDT
From: koonin@sbitp.bitnet
Message-Id: <890517101432.162c@sbitp.ucsb.edu>
Subject: RE: Cold Nuclear Fusion library for our panel.
To: rlg2@yktvmv.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet"

Dick: I've no problem with your passing my bitnet messages on to others on the panel. Please do so if you're set up for it.
Steve

Date: Wed, 17 May 89 10:20:19 PDT
From: koonin@sbitp.bitnet
Message-Id: <890517102020.162c@sbitp.ucsb.edu>
Subject: RE: Microcracks.
To: rlg2@yktvmv.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet"

Dick: Large fields in an insulator I can buy, but not in a conductor. I seem to recall that the Pd/D system has a conductivity almost as good as copper! Perhaps there's an insulating surface layer (oxide?) that builds up and produces a field.

Steve